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COMMONSENSE AND CONVENTIONAL WISDOM

TIMOTHY D. HACKENBERG

UNIVERSITY OF FLORIDA

Staddon's paper contributes to an ongoing dialogue concerning the proper interpretive focus of the type of work published in this journal, a dialogue that spans at least the past four editorships (see Branch, 1992; Hineline, 1984; Nevin, 1980). Although decisions regarding publication of manuscripts hinge primarily on methodological matters, such as demonstration of experimental control in individual subjects and reliability of results through replications, there is also an implicit interpretive framework, referred to in the present context as "environment-based theory," that much of the work in this domain shares. Environment-based interpretations are distinctive not only in their emphasis on environmental determinants of behavior but also in their departure from conventional locutions of psychological explanation. Moreover, many of the crucial features of environment-based theory are subtle and difficult to parse in everyday terms. Thus, debates like the present one, which provide an occasion for an explicit statement of some of these key features, are exciting and important. Although I disagree with several of the ways in which environment-based theory is characterized by Staddon, his comments are nevertheless useful in forcing a reexamination of some of the assumptions upon which it rests, assumptions that too often are either unacknowledged or are defined only negatively, for instance, as standing in opposition to "mentalistic," "cognitive," or other variants of organism-based theory.

According to Staddon, environment-based theory is atheoretical, guided by a kind of crude induction, and ahistorical, with variables acting in a vacuum. I will attempt to show that this characterization is applicable neither to current environment-based theory nor to its historical antecedents. But Staddon's paper is more than just a critique of particular features of environment-based theory; it is also a critique of the verbal practices that maintain such theory. Staddon regards these practices as overly confining and exclusionary; he believes it is time to relax the contingencies on explanatory talk, partly to achieve greater integration across disciplines. The main purpose of the experiments and simulations discussed by Staddon (pp. 442–446) is to illustrate some common ground that is shared by organism-based and environment-based modes of theorizing, thus to assert that the distinction between them is arbitrary. But rather than revealing common ground, the material in that section of the paper actually reveals deep and perhaps incommensurable philosophical differences between the two positions—differences regarding the nature of explanation and the role social circumstances play in shaping such explanations. In short, Staddon's illustrations show that these aspects of the way we talk about our data do matter.

Organism-based theory is heir to a philosophical tradition dating back to at least Descartes, and extending through to the present day in computational information-processing models of cognitive psychology. Though varying in detail, theories in this tradition all give a special explanatory role to events within the organism. These sometimes are construed as physiological processes, but often they are purely metaphorical; in either case, an organism's behavior is seen mainly as an index of these internal states, which form the basis for causal statements. The outside world is not experienced directly, but rather indirectly through copies, or representations, of that world. The *re*presentation of past events bridges the gap between the original occurrence of those events and current behavior, revealing another prominent feature of most organism-based interpretations—a commitment to proximal modes of causation. Thus, in the cumulative trace (CT) model, to account for the declining effectiveness of remote reinforcers, Staddon invokes the concept of a memory trace, which links past events to current behavior in a con-

Correspondence should be addressed to Timothy D. Hackenberg, Department of Psychology, University of Florida, Gainesville, Florida 32611.

tiguous causal chain. This appears to be the primary function of the trace, for Staddon himself acknowledges no formal difference between this model and the cumulative effects (CE) model. Organism-based theory *must* invoke the trace, or some such concept, in accounting for past events because that is the kind of theory it is.

Environment-based theory has quite different philosophical origins, and therefore quite different assumptions about the nature of the subject matter and the goals of a scientific theory. It takes an organism's activity in the world as a starting point; not as a manifestation of some deeper psychological process, but as process worthy of study in its own right. This activity forms the basis for its scientific concepts. One key concept is that of an operant class, a generic collection of responses defined through natural regularities with the environment. As an analytic unit, an operant class closely resembles the concept of species in biology; both are inherently abstract, insofar as the members that comprise either unit are never all present at once, but they are also real and tangible, insofar as those members are real-life measurable events. Implicit in the definition of an operant is an ongoing environmental context; thus, an operant is not merely a unit of behavior, but rather is a contextual unit of behavior–environment interaction dispersed in time and place. Environment-based theory therefore is not limited to contiguous modes of causation, although it is often mischaracterized as such by those who read "environment-based" as including only stimuli immediately surrounding isolated instances of behavior.

Because the causes of current behavior are said to lie in past behavior–environment interaction rather than in intervening events, environment-based explanations are necessarily historical. Staddon's comments to the contrary are puzzling. Granted, in the laboratory the effects of remote histories are often masked by more current variables when those effects are not the primary focus of investigation; specific histories have been examined, however, as topics of interest in their own right (Wanchisen, Tatham, & Mooney, 1989; Weiner, 1964). But even when the focus of investigation is on current variables, environment-based interpretation looks to past history—not only the history of the individual but also the history of the species (and in the case of human behavior, of the culture) of which the individual is a member (Skinner, 1981). In fact, in its rejection of mediating variables as surrogates of past history, a strong case could be made that environment-based theory is actually the closest psychology comes to genuine historical explanation, thereby linking it with historical explanations in other sciences, such as paleontology (Gould, 1989), evolutionary biology (Dawkins, 1986), cultural anthropology (Harris, 1979), adaptive network models (Palmer & Donahoe, 1992), and nonlinear dynamics (see the special issue of *JEAB,* 1992, Vol. 57, No. 3). Because initial conditions are imperfectly known, historical explanations strive for sufficiency (specifying the historical paths that might have produced a particular outcome) rather than necessity (specifying the only necessary path) (see Donahoe & Palmer, 1989); this appears to be the same point Staddon makes with his notion of "equivalent histories" (pp. 444–446).

From the perspective of a historical approach like environment-based theory, the concept of a memory trace is never even raised, because the approach accepts causes extending over time as a basic and nonmysterious fact. Thus, a shift of time scale need not entail a shift to a different kind of explanation, as implied in Staddon's assertion that the CE model is environment based whereas the CT model is organism based. The primary difference between these two models is that the latter model better specifies the effective environment in its interplay with behavior. Questions about memory traces or other intervening events, even if linked to measurable entities, appear misguided or badly framed. Analogously, from the standpoint of organism-based theory, descriptions of behavior–environment interaction, even if systematic and coherent, may not sound very explanatory. In short, these two are different kinds of theories, founded on different assumptions, asking different questions, and accepting different answers. It is not simply a matter of translation, for the terms and concepts of one theory will appear awkward or trivial within the context of the other.

From an organism-based perspective, the concepts of environment-based theory may not appear theoretical at all. This is apparently the conclusion reached by Staddon, citing Skinner's (1950) much-heralded critique of theory. But the target of Skinner's criticism was not theory per se (for he was openly advancing a theory of a different sort) but a par-

ticular brand of logical-deductive theory in psychology, of the sort that typifies current organism-based interpretation. Hypothetico-deductive methods have proven successful in other sciences (a point Skinner repeatedly acknowledged) but have had unfortunate consequences when joined with assumptions that form the basis of organism-based theory.

Among the most problematic assumptions upon which organism-based theory rests is the implicit assumption, dating back to Socrates, that the formal structure of a theory corresponds with the formal structure of the world. It assumes further that the subject matter of a science can be formalized as a set of abstract rules. These base assumptions are discernible in Chomskian linguistics (Chomsky, 1980), in mediational theories of memory (Watkins, 1990), and in formal computational models of artificial intelligence (Dreyfus & Dreyfus, 1988). This raises no serious problems as long as the subject matter of science is conceptualized as a collection of context-free facts. But the problem, as Dreyfus and Dreyfus point out, is that the everyday world (which forms the background skills and discriminations of the scientist) has no theoretical structure, at least not one that can be formalized into abstract rules. Instead, the everyday world gives rise to practical skills of effective action, sometimes more intuitive than rational, that simply resist specification in purely formal terms. The endless task of formalizing this background understanding of the real world is what Dreyfus and Dreyfus call the problem of *commonsense knowledge,* which is largely responsible for the demise of formal computational models of artificial intelligence and the rising popularity of more contextual models, such as adaptive networks.

This general line of argument finds a home in environment-based theory. A distinctive feature of Skinner's position was his insistence that knowledge, including scientific knowledge, is rooted in the practical circumstances of everyday life. (This general orientation owes much to Mach, whose influences on Skinner ran far deeper than just a blind commitment to inductive science, as implied by Staddon; see Chiesa, 1992, and Marr, 1985). Commonsense knowledge, as Dreyfus and Dreyfus point out, "amounts to what might well be *everyday know-how.* By know-how we do not mean procedural rules but knowing what to do in a vast number of special cases" (p. 33). Skinner (1969) similarly rejected the implication that everyday contingency-governed behavior was supported by a background of formal rules: "When an organism is brought under the control of complex contingencies, it is not necessarily 'applying the rule' which describes them" (p. 81). Rules can sometimes be extracted from such circumstances, and even codified into methods, but they are not formal rules of deductive logic, but rather are practical rules of effective action that specify or imply contingencies of reinforcement. Deductive logic has its place, but it is not the privileged place it occupies in traditional theory. There is also room for what Staddon calls "invention" and "discovery," although such terms, borrowed from everyday speech, gloss over important distinctions arranged by the scientific verbal community. The effort to maintain a coherent technical vocabulary is merely a recognition of the fact that scientific talk, founded on practical circumstances, is subject to the contextual determinants of everyday language. Because it is impossible to "decontextualize" scientific activity into a set of procedural rules, as implied by strictly formal analyses, an effective formulation of scientific conduct waits on an analysis of the verbal behavior of the scientist and of the community that gives rise to and maintains such behavior.

Even if one backs off from the strong organism-based view embodied by formal computational models of cognition and adopts instead the more pragmatic view of theory as heuristic device, it remains to be seen if organism-based theory can bring its own terms and concepts to bear on itself, as environment-based theory, following Skinner's (1945, 1957) lead, has done in recent years (Hineline, 1992; Moore, 1987; Schnaitter, 1978). Or, will organism-based theorists be "mentalistic about the causes of their own behavior when they explain their own behavior of explaining" (Moore, 1983, p. 5)? This is part of the commonsense knowledge problem that has already left computational models of artificial intelligence in its wake. Can the rest of the organism-based enterprise be far behind?

REFERENCES

Branch, M. N. (1992). On being narrowly broad. *Journal of the Experimental Analysis of Behavior,* **57,** 1–4.

Chiesa, M. (1992). Radical behaviorism in scientific frameworks: From mechanistic to relational accounts. *American Psychologist,* **47,** 1287–1299.

Chomsky, N. (1980). *Rules and representations.* New York: Columbia University Press.

Dawkins, R. (1986). *The blind watchmaker.* New York: Norton.

Donahoe, J. W., & Palmer, D. C. (1989). The interpretation of complex human behavior: Some reactions to *Parallel Distributed Processing,* edited by J. L. McClelland, D. E. Rumelhart, and PDP Research Group. *Journal of the Experimental Analysis of Behavior,* **51,** 399–416.

Dreyfus, H. L., & Dreyfus, S. E. (1988). Making a mind versus modeling the brain: Artificial intelligence back at the branchpoint. *Daedalus,* **117,** 15–43.

Gould, S. J. (1989). *Wonderful life: The Burgess shale and the nature of history.* New York: Norton.

Harris, M. (1979). *Cultural materialism: The struggle for a science of culture.* New York: Random House.

Hineline, P. N. (1984). Can a statement in cognitive terms be a behavior-analytic interpretation? *The Behavior Analyst,* **7,** 97–100.

Hineline, P. N. (1992). A self-interpretive behavior analysis. *American Psychologist,* **47,** 1274–1286.

Marr, M. J. (1985). 'Tis the gift to be simple: A retrospective appreciation of Mach's *The Science of Mechanics. Journal of the Experimental Analysis of Behavior,* **44,** 129–138.

Moore, J. (1983). Introductory remarks. On cognitive and behavioral orientations to the language of behavior analysis: Why be concerned over the differences? *The Psychological Record,* **33,** 3–6.

Moore, J. (1987). The roots of the family tree: A review of four books on the history and nature of behaviorism. *The Psychological Record,* **37,** 449–470.

Nevin, J. A. (1980). Editorial. *Journal of the Experimental Analysis of Behavior,* **34,** 133–134.

Palmer, D. C., & Donahoe, J. W. (1992). Essentialism and selectionism in cognitive science and behavior analysis. *American Psychologist,* **47,** 1344–1358.

Schnaitter, R. (1978). Private causes. *Behaviorism,* **6,** 1–12.

Skinner, B. F. (1945). The operational analysis of psychological terms. *Psychological Review,* **52,** 270–277.

Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review,* **57,** 193–216.

Skinner, B. F. (1957). *Verbal behavior.* New York: Appleton-Century-Crofts.

Skinner, B. F. (1969). *Contingencies of reinforcement: A theoretical analysis.* New York: Appleton-Century-Crofts.

Skinner, B. F. (1981). Selection by consequences. *Science,* **213,** 501–504.

Wanchisen, B. A., Tatham, T. A., & Mooney, S. E. (1989). Variable-ratio conditioning history produces high- and low-rate fixed-interval performance in rats. *Journal of the Experimental Analysis of Behavior,* **52,** 167–179.

Watkins, M. J. (1990). Mediationism and the obfuscation of memory. *American Psychologist,* **45,** 328–335.

Weiner, H. (1964). Conditioning history and human fixed-interval performance. *Journal of the Experimental Analysis of Behavior,* **7,** 383–385.